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An

Inaugural Essay

on

The state of the biliary secretion in fever,

For the degree of Doctor of Medecine

In the University of Pennsylvania.

By

Wm W. Waddel, of Georgia.

Philadelphia,

January 1st

1828.

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The arguments advanced in the following essay are founded on an opinion opposed to the general belief, that the fevers of the Southern country are bilious in their character. In proving the position, the author will be obliged to contend with popular prejudice rather than Medical authority properly so called; for it may be remarked, that it is the mass of common people who be-believe the doctrine & not the scientific Physicians who promulgate it. Many professional men, no doubt, have fallen into the prevailing opinion that overplus or depravity of bile, exercises much influence in our fevers; but something has hitherto prevented them from accounting for, or proving it. In essays on fever, the subject is occasionally introduced, but if we may judge from appearances, it has only seemed to claim attention in consequence of the universal belief in its powerful agency & not from any conviction in the minds of the authors that this agency was correctly imputed to it. It is not pretended that the arguments here brought forward, are altogether

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the result of observation; but they will not be condemned by the enlightened reader because proofs have not always been received through the medium of the external senses. Our senses are known to be imperfect, & in the investigation of subjects that require their minute employment & accurate discrimination in so great a degree as Physiology & Pathology, we arrive at a point where they totally fail us. Here we draw on the resources of our reasoning powers, for well we know that many things are demonstrable to reason that are not to sense. Reference to a principle in Natural Philosophy will prove this. In endeavouring to detect the ultimate particles of matter by the aid of the microscope, they become so minute as to be confounded with the surface on which they are placed. At this point the power of the organ of vision assisted by the art of man, stops; but reason goes many steps farther & tells us, that matter is infinitely divisible.

Hitherto, whenever the agency of bile in fever has been disputed, the author of such an opinion has been accused of confounding cause & effect. To free the present essay from such an imputation, three positions are assumed

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as all equally true. 1st That super-abundance or depravity of bile. is not the cause of fever. 2nd That it is not the effect of fever. 3rd That it is not directly or indirectly the effect of any of the usual causes of fever. To prove first, that it is not the cause of fever, we should investigate the physiological effect of the usual quantity of healthy bile on the system & thence form an opinion of the probable pathological effect of an increased quantity or morbid quality of the same fluid. We are taught that it is the office of one portion of the bile, to assist in the separation of the chyle from the chymous mass brought into the duodenum, & that the office of another portion is, to stimulate the alimentary canal & cause it to move forward its contents. The latter is now a settled point in Physiology. It is proven that bile contains a purgative principle by the experiments of Professor Chapman on the bile of inferior animals, which he found actively cathartic. Negative evidence will also be found in the fact, that in those diseases where external symptoms indicate deficiency of biliary secretion, there is always

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torpor of the intestines & obstinate costiveness. This is exemplified in Jaundice, Dyspepsia &c. Having those known effects from healthy & deficient secretion, our most natural corollary would be, that super-abundant secretion would purge or produce Diarrhea. Acrimony or depravity of bile has been ascribed to the influence of solar heat, where, by the profuse perspiration which takes place, an abstraction & reabsorption of the more dilute element is occasioned. It is to be presumed that the bile subjected to such a process, must acquire a concentrated degree of power & that its irritant & corrosive effect on the mucous membrane of the primae viae would be greater. According to the intensity of the irritation, we should then expect a violent Diarrhea or a mild or a severe case of Dysentery. If bile in a state of overplus or depravity, be the cause of our fevers, we may hence legitimately conclude, that those fevers would be preceded by, ushered in or exist cotemporaneously with, Diarrhea or Dysentery. But those, far from being common, are very unusual symptoms. The converse of this will be found partly true, namely - that Idiopathic

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Diarrhea is not attended by fever as a necessary symptom. It is not therefore probable that the fever called the Bilious, is ever caused by those states of the bile.

The second position assumed is, that overplus or depravity of bile is not the effect of fever. It would be transcending the bounds of the present essay very far, to attempt an exposition of the effect of all the phenomena of fever on the biliary organs, & there is no symptom which could be considered a proper representative of their aggregate. We may, however take one of the most common, viz. universal oppression followed by arterial reaction. It is agreed by all, that in the first mentioned state there is general diminution of secretion. Let it be supposed also, that when reaction & arterial excitement take place the secretory vessels will return to the execution of their proper functions in most of the glandular apparatus. But the liver will always be an exception to this rule, for it derives the materials of its secretion from its veins instead of its arteries. By the augmented action of the arteries, those veins become engorged with blood & this fact

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has been demonstrated by Physicians who strenuously advocate the bilious character of our fevers, by reference to the minute intertexture & close ramification of the hepatic arterial system with the venous system of the same viscus, whereby, an encroachment on the calibre of the latter is rendered easy. The veins are certainly destitute of any reactive power to overcome this morbid distention & obstruction, therefore circulation will be partially, and secretion totally suspended.

But the most impartial view that can be taken of this subject will be to prove the third position assumed, or in other words, to trace the common causes of fever in their influence on the animal economy & see whether they can be so modified in their application to the system, as to be productive of increased or morbid biliary secretion. This will in a great measure include all that it will be necessary to say in a separate consideration of the symptoms; for if we follow the causes to their ultimate results, we will certainly, at some stage of our progress, come across the most prominent symptoms. Among the prominent causes

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of fever, marsh effluvium presents itself. The manner in which it is applied to, or enters the system is yet a contested point. It is believed by some that it is applied to the skin, that it paralyses the capillary vessels, & that the circulation being thus obstructed in them, is confined to the deep-seated, internal vessels; the latter then make an effort to disburthen themselves of their surplus load, but, owing to their overpowered & debilitated state they are unable to succeed, & the fruitless effort is called fever. For arguments sake, let this theory be admitted as true. The veins & especially those that supply the larger viscera, would here become engorged from two sources. 1st They would receive their proportionate quantity of the blood confined to the internal parts, by the paralysing effect of the miasmata on the surface & 2nd they would have an unusual quantity forced into them by the increase of arterial action which always takes place in fever. As they are not capable from natural construction, of sending forward their contents in a ratio to be compared to the arteries, they are obliged to submit to the engorgement.

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Under those circumstances, it is impossible that those veins from which are derived materials for the secretion of bile, can in their engorged & disabled state, furnish elements for an unusual quantum of that fluid. We are taught by Physiologists that the blood, before it arrives at a gland must undergo some preparatory changes, which predispose it to furnish the elements of its peculiar secretion. But can this prefatory process take place when the blood is forced so rapidly into the veins by the power of arterial excitement? If it does not occur before it arrives at the point of secretion in the liver, we cannot expect it to take place after it gets there. In the latter case the original ability of the blood to effect those preparatory changes or its former susceptibility of having produced on it, such changes, is most probably taken away, because it has become morbid in character in consequence of having assumed an unnatural, stationary position. It is necessary to the healthy character of the blood, that the oxygen inhaled should carry off the carbon, but this process cannot take place when the blood is stopped

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in its course by engorgement in the vessels of a large gland & thus prevented from passing through the lungs. Physiology recognises no other point at which the blood can come in contact with oxygen, the carbon must then remain & that part of the blood become diseased. It will of course no longer furnish pabulum for secretion.

A new theory of the modus operandi of miasmata was published in Charleston in 1820. It was supposed that they entered the system through the medium of the function of respiration; that being a combination of noxious gases, they acted only by virtue of their mass or bulk, by which, in a mechanical manner they excluded the necessary quantity of oxygen from entering the lungs. That proportion of carbon which is generated in the blood between the time of its departure from & its re-entrance into the lungs, being thus detained in it by the intervention of a third agent, (miasm,) which prevented it from exposure to oxygen, it, (carbon) was supposed to produce the same effect on the blood as so much milk, ether, alcohol or any foreign substance, injected directly into the bloodvessels.

If this theory be true, the blood would be in a positive state of disease. But a healthy condition of the blood is necessary to supply the usual quantity of materials for secretion & in its diseased state, there will not be an increase but a diminution of the same function.

It is believed by one class of theorists that marsh miasmata enter the system through the olfactory nerves. Dr Rousseau demonstrated that their action was transmitted to the stomach by means of the intimate connexion existing between all the parts composing the mucous membrane. The nervous expansion of this tissue, would be of course, the principal agent in conveying the poison. But can we suppose that the nerves, in the performance of this imputed office, will remain free from a direct state of disease themselves? Can they be the messengers which carry disorder & death to every other part of the system & yet be unaffected in their own delicate structure? Certainly not; for if marsh miasma has any positive existence (which eudiometry does not prove) it must diminish & possibly paralyse the energy of the nerves when applied to them.

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It is a beautiful doctrine in Physiology which teaches us that secretion depends for its immediate cause on an intuitive perception in the nerves, by which they discover that the materials for secretion are passing along in the bloodvessels & are ready to be drawn out. As soon as this discovery is made & communicated to the secerning agents, they act on the impulse & the work of secretion takes place. Rejecting the idea of any mechanical agency in a process so purely vital, & assuming the nervous influence as the efficient cause of secretion, it becomes evident that any interruption of that influence must cause a corresponding derangement in the action of the secreting powers of the glands. According to the present theory of the modus operandi of miasmata, the energy of those nerves subservient to the secretion of bile is directly diminished, (the choledic duct & glandular acini being lined with a continuation of the same membrane on which the miasmata first act) therefore secretion from the liver will be suspended & not increased. At first sight, this view of the case will appear directly opposed

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to the law laid down by Bichat, that irritation developed on the mouth of a duct, will be transmitted to the gland from whence that duct proceeds & will cause secretion in it. But it remains yet to be proved whether the action produced by miasma is properly an irritation. The fact that endiometry cannot detect its existence, gives us reason to believe that the effects usually attributed to it, are owing to the subtraction of some healthy stimulant from the medium in which we live, rather than to the presence of an unhealthy one, capable of producing irritation.

It may not be improper here, to make a short digression. Inasmuch as it is presumed that the action of miasmata on the nervous expansion of the mucous tissue, tends to deprive them of their sentient & inherent power of discriminating the materials for secretion in the bloodvessels, & as those materials are obliged consequently to remain & go on in the circulation, &, as we know from analysis that the constituent elements of bile are such as are very unfavorable to health, may not their detention in the vena portarum be the cause

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of some of the anomalous phenomena of fever not yet accounted for? The presence of bile in the blood has been denied, but its elements may certainly exist & not be cognisable to the senses when we presume that we have made the most impartial experiments. The conception of such an idea does not insinuate a belief in the doctrines of the humoral pathology. There is no primary disease of the fluids alluded to, but only a disordered condition of those fluids, which is the sequel of a morbid action first developed on the solid. The following quotation from the writings of Dr S. Jackson will support the supposition. "Is it not probable, that in protracted diseases, attended with interruption of secretion & excretion, digestion & healthy nutrition, the quality of the blood may become depraved to a certain extent, so as to become incapable of a healthy restoration to irritability?"

It is the creed of Prof. Chapman that miasmata as a cause of disease, become entangled in the food or saliva & thus enter the stomach. It would cause

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but a slight modification of disease whether the primary impression were made on the stomach or any of the arbitrary anatomical divisions of the intestines; the mucous tissue which lines the whole is exposed to the front of the battle & no doubt engrosses to itself the influence of the morbid agent. But it has been shewn that miasm acting on this tissue cannot result in overplus of bile.

Some experiments have been made to prove that miasmata are taken into the system by pulmonary & cuticular absorption. The authors of the most conclusive of those experiments, tell us, that their only test of the presence of the deleterious matter in the system, was its discovery in the blood & some of the excretions. Under these circumstances, (allowing their theory to be correct) it must have affected the circulation directly, in the manner formerly alluded to & it has been proved that we could not rationally expect increased secretion of bile to be the consequence. A general conclusion may then be drawn from what has been brought forward, that marsh effluvium as a cause of fever, cannot

from any method of its application, either directly or indirectly, influence the portal circulation to excessive elaboration of bile.

There are other causes said to produce fever & among them, solar heat is frequently mentioned. The ultimate effect produced on the system when subjected to the power of this agent, is atony of the extreme vessels from the previous great excitement & this results in a destruction of the balance of the circulation. In this state of things it is the venous system alone which will suffer & it has been demonstrated that during the existence of engorgement or congestion in them, we cannot rationally expect undue secretion of bile. But there is a negative proof that heat does not have such an effect, for if we attribute such results to it, we virtually ascribe them to the influence of an universally pervading cause. In a country (such as Carolina or Georgia) where this cause acts universally we should always & in every place be afflicted with those diseases resulting from overplus of bile (Diarrhea, Dysentery &c.) But this is not the case. There

are many places where no diseases, solely attributable to peculiarity of climate are ever known; spots that are proverbially healthy, so great is their celebrity that they are thronged every summer with the ranks of wealth and fashion, flying from the miasmatic neighborhoods of St Marys, Darien, Savannah, Charleston &c. The places alluded to are not attended with any of the collateral advantages of mineral springs &c. but are attractive because essentially healthy & salubrious.

The agency of heat in the production of increase of biliary secretion, may be almost looked upon as a settled doctrine in pathology. Among scientific men, the opinion owes much of its popularity to the support it derives from the celebrated Johnson whose writings are so much read in the United states. In the present essay it has not been doubted that heat may vitiate the bile by causing a reabsorption of the serosities. But sufficient reasons for denying the existence of that process as preparatory to, or cotemporaneous with our febrile diseases, have been already stated, viz. the absence of the first symptoms, (Diarrhea, Dysentery &c.)

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that would most probably follow according to rational prognosis. Let us more closely examine whether solar heat, has any peculiar power of generating a redundancy of bile. Dr Johnsons views are founded on a belief in the existence of a sympathy or synchronous action between the vessels on the surface & those in the liver, whereby a cause affecting the former will be propagated to the latter, or in other words, on the application of heat & consequent increase of perspiration, there will be a sympathetic increase of bile. In studying the character of this theory, two or three axioms in Physiology should be kept in view. 1st The blood affords the pabulum for all the secretions & depurations. This fact has the support of the most eminent Physiologists, Hunter, Bichat, Broussais &c. 2nd There is but a definite quantity of this fluid in the animal economy; & from it as a grand reservoir, all the secretory glands & depurating emunctories, abstract a part for their own proper purposes. 3rd When on the application of stimulus, any one of those glands or emunctories is excited to increased action, there must be a proportionate diminution of action in every other, but more

especially in those connected with the excited organ by sympathy. If the second axiom be true, (that there is but a definite quantity of fabulum,) & we suppose the already large portion of the mass carried off by the skin, to be augmented, shall we not presume by consequence that every other gland & emunctory will secrete or carry off less? Most certainly! But if the third axiom be true, (that those emunctories & glands connected by sympathy act as antagonist organs to each other) shall we not by a still more legitimate deduction, infer, that increase of perspiration will cause decrease of bile? Certainly, because the skin & liver are intimately connected by sympathy. We are well aware that increase of urine will cause decrease of perspiration & vice versa; but according to Dr Johnsons theory we are not to be content with supposing that the diminution of perspiration in Diabetes, is sufficient to balance the increase of urine, but must also suppose deficiency of bile in the same disease. Do reason & observation warrant us in believing or does experiment prove, that the quantity of urine carried off by the kidneys, is equal

to the combined sum of the cutaneous depurations & hepatic secretions? A quotation from the philosophic Bichat may render the general position here assumed, more tenable. "Heat which expands the cutaneous system, increases its action at the expence of that of the glands & reciprocally cold which contracts it, forces the glandular system to supply this action."

But a very prominent objection drawn from every days observation may be urged against Dr Johnsons theory. If it is an invariable law of our economy that there shall be an increase of bile on the increase of perspiration, why is it that labourers, who are always in a profuse sweat are free from diseases resulting from increased secretion of bile. To account for the absence of Diarrhea in these cases, we are told by Dr Johnson that nature establishes a vicarious sympathy in the mucous membrane, by which it is caused to secrete less mucus & the intestines being consequently destitute of their natural lubricity, do not so readily yield to the purgative influence of the bile. But if they do not yield to the purgative, will they not, when

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deprived of their mucous defence, more easily suffer under the corrosive influence of the "boiling lead."? Contrast Dr Johnsons opinion on this subject with that of Dr Cleghorn. "Sharp, indigested humours, flowing from the stomach, or (which is more frequently the case) an increased secretion of bile & other liquids necessary for chylification, produce a simple looseness, which gradually washes off the mucus from the intestines, erodes their membranes & at last brings on severe gripes with bloody stools." This paucity of mucus which Dr Johnson thinks necessary to prevent Diarrhea, will according to Dr Cleghorn, cause Dysentery in its worst form. But labouress do not have either disease. Therefore Dr Johnsons theory is improbable. He refers to some experiments made by Bichat which proved during the time of digestion in the stomach, that the biliary secretion was diminished, but as soon as the food passed the pylorus, that it was increased. He traces the same analogy here, asserting that in the first part of the process, the skin is dry but when the food gets into the duodenum, it relaxes & perspiration is much increased. Suppose the fact perfectly established, that the liver pours its bile

into the duodenum immediately on the passage of the chyme through the pylorus, how are we to detect the exact moment so precisely, as to say whether or not there is an increase of perspiration by synchronous action. We cannot tell from mere sensation. Digestion is a perfectly insensible process & no one but the dyspeptic, whose nerves are endowed with a morbid acuteness of sensibility, can ever tell when the food passes through the pylorus. This then, is not a legitimate proof, nor would it ever be recieved as such.

To multiply a list of the causes of fever & explain their mode of action, would be but to repeat the body of the argument already gone over. The uniformity of the symptoms of our fevers, proves, that cold, moisture, or both combined, or acrid ingesta &c. all produce the same ultimate effects as miasmata & solar heat; the most cognisable of which (to the senses) is, destruction of the balance of the circulation. But it has been proved that such a state cannot result in overplus or depravity of bile.

Inasmuch as this state of the biliary secretion does not exist as the cause or effect of fever, nor as the indirect result of

any of the causes of fever, it certainly does not exist at all in connexion with febrile disease. It is an error that has been copied by one physician from another, until it is sanctified by age, it is now deemed heterodox to depart from it merely from the force of early impressions, & finally, it never had any existence, it is a monster, a bug bear created in the imagination of some misled pathologist of former times.

There are some signs which are considered always diagnostic of a bilious habit, but they can be proved to be illegitimate & false on a very slight examination. 1st It is said that in our fevers, we have dark, tarry or particoloured stools which are known to consist of vitiated bile. In regard to this let us ask if they are the natural attendants of a fever? Are they ever seen previous to the exhibition of calomel? May it (calomel) not by its energy (which we are told is principally expended on the liver) extort an unusual quantity of bile? Those questions answer for themselves. But should it be asked whether calomel can induce a condition of the bile so vitiated, the answer is, that it is possible & even probable that it can. This is a

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point which cannot be tested by experiment & must remain always problematical. We may procure pure & healthy bile & genuine calomel, mix them & apply a temperature equal to that of the alimentary canal, but observation made on such an experiment would throw no light on the reciprocal action of bile & calomel on each other in the alimentary canal. We might add to the mass, the gastric & pancreatic fluids, & suppose the peristaltic motion & the action of the different glands, to be severally unnecessary to perfect the process. yet there would still be a "je ne sais quoi," an indefinite something, wanting to render our deductions correct. Chemistry with all the light it has thrown on modern science, cannot conduct us into the dark caverns & recesses of the human system, nor shew us the modus operandi of the thousand invisible agents concerned in the functions of the animal economy. It is certainly rational to suppose that the vitiated appearances in the excretions of febrile patients may be the result of some recondite combination of the fluids of the primae viae with bile & calomel. Substitute "quid pro quo," any

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other medecine & we shall still be in doubt whether a combination of the same kind may not take place.

But a case may be supposed where the state of the excretions above alluded to, may exist, independent of the exhibition of any purgative. Dr Daniell of Savannah tells us that the evacuations which followed his application of sinapisms to the abdomen were very offensive. To account for this without the necessity of supposing the bile to be primarily predominant or vitiated, it is only necessary to refer to the true state of the biliary secretion in our fevers. That it is diminished, is proved by the constipation always present, it is negatively proved by every argument in the former part of this essay, & high authority can be quoted to support the belief. "Contrary" says Prof. Chapman, "to the prevalent notion, in the fevers emphatically termed bilious, there is a great deficiency of bile sometimes, the liver being so torpid, either from the force of the remote cause, or by early congestion, that its secretions are suspended, and, until their restoration, we advance slowly with the cure". On the application of a sinapism the portal congestion is

relieved, the circulation is equalised & the blood from which the elements of bile are derived being restored to comparative health, the secretion will return with violence & impetuosity of action & abundance of quantity, proportioned to the time of its suspension. From detention in an unnatural situation, it is also probable that it will have acquired its vitiated appearance & offensive character.

2nd Yellowness of the eyes is looked upon as one of the signs that a man is "bilious." (This phrase always implies predominant secretion.) This symptom is often said to exist, when it cannot be percieved unless forsooth, the physician takes the trouble to press down the lower eyelid, when a yellow tinge can be seen, which, nine times in ten is apparent in perfect health. Dr Fordyce ascribes it to an overplus of the sebaceous matter, natural to the part. Dr Daniell thinks it the result of diseased state of the capillary vessels. But let us admit that it is bile which causes it. So far from indicating that the secretion of that fluid is excessive, it merely points

out to us, that the usual quantity is obstructed in its passage to the duodenum by some pathological state of the ductus communis or the gall duct proper. It is the cystic bile (if it is bile at all) which is diffused over the system, whether it be from absorption or regurgitation. But the manner in which the yellow appearance is explained by Drs Daniell & Fordyce, appears infinitely more rational.

3rd It is contended that as a precursor of our common autumnal fevers we have a vomiting of bile. This does not prove however, that there is any thing peculiar connected with the biliary secretion. Among those causes that produce fever, there are some whose primary effect is, to nauseate & cause vomiting. This nauseating cause may be so extended as to invert the peristaltic motion in the duodenum; hence the choledic duct may be emulged & bile in a natural state being brought into the stomach will be readily ejected. If it be more decided in colour or bitter in taste than natural hepatic bile, we might readily suppose that the gall bladder (which, owing to the contraction of the abdominal muscles in vomiting, is

pressed upon by the surrounding viscera,) had also evacuated its contents. If we were called upon to give a correct name to the mass thus vomited, it would be wrong to call it "bilious" matter, even though we had reference only to natural bile. Dr Fordyce suggests on this subject that we are sensible of the presence of bile because it is rendered conspicuous by its characteristic properties of bitter taste & smell & decided colour, while the gastric & pancreatic juices (being no doubt also present) are deficient in those sensible properties. The choledic duct opens into the duodenum, & were bile superabundant or acrid it is more probable that it would produce its natural effect, (Diarrhea or Dysentery,) than that it would by sympathy nauseate the stomach & produce vomiting. The one, we can explain by referring it to actual contact & direct action, the other can only be accounted for through the medium of the more mysterious agency of sympathy.

It may be objected to the essay now closed, that if it were all true, it answers no practical end, or in other words, if overplus or depravity of bile, have no connexion

with fever, deficiency of the same fluid may still have great agency in the disease, & that the term "bilious" & the mercurial practice will apply equally well to both. The term is objectionable & should not be used, because, on its very "head & front" it will always appear to imply overplus. In regard to the unity of the practice in deficiency, redundancy or depravity of the fluid, the enlightened physician need scarcely be reminded, that, although the same medecine may be used advantageouly in all the cases, yet the mode of giving it in each, is quite different, & the different methods are succeeded by widely different results. In a case of overplus of bile, (were it possible for it to be too insipid to carry itself off,) the largest dose of calomel is given, followed by laxative medecine; only extending its influence so far as to purge the primae viae & carry off the offending matter; but in deficiency of bile, arising from torpor or congestion of the liver, the calomel is given in small doses, repeated at certain intervals, to secure a stimulant effect on the sluggish apparatus by causing its power to accumulate gently & gradually. The secretions are thus solicited, not extorted, & the patient conva-

lesces without that afflicting & permanent derangement of the digestive function, so often consequent on the exhibition of large & powerful doses of medicine. That this derangement is often the sequel of a mercurial treatment, we are easily persuaded to believe, when we reflect on the cruel & unnecessary waste of the gastric, pancreatic & biliary fluids which it occasions, the high grade of excitement to which the organs which should respectively furnish those fluids, are stimulated, & the necessarily subsequent debility & collapse to which they are subjected. This practice especially demands reform in Carolina & Georgia, where calomel is the fac totum in fever.

The foregoing remarks have been offered because it has occurred to the author that the subject, though much hackneyed, was important. If the doctrine shall be proved incorrect, no harm is done, but the base on which the present wide spread belief rests, shall be sensibly widened, & the superstructure of course more firm

Dr Chapman

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